Reply to comment by F. H. Cornet on 'Large-scale in situ permeability tensor of rocks from induced microseismicity'

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1 INTRODUCTION

We appreciate Dr Cornet's interest (Cornet 2000) in our paper. Before starting with a direct reply to his comments let us first make a few preliminary remarks.

Seismic methods have some fundamental difficulties in estimating hydraulic properties of rocks such as the fluid mobility or the permeability tensor (see e.g. Shapiro & Müller 1999 for references related to this problem). The main results of our paper (Shapiro *et al.* 1999; SAR99 hereafter) are the principles of the passive seismic-based method for *in situ* estimation of large-scale permeability tensors in rocks, which are developed in Sections 1–3 of SAR99. In the following we will call the method SBRC: seismicity-based reservoir characterization. This method can be applied to microseismicity clouds induced by fluid injections of various kinds, not just by hydrofracturing in boreholes.

As we understand it, Dr Cornet concentrates his criticism on the application of the SBRC to the case of hydrofracturing. We disagree with his arguments and think that cases of hydrofracturing injections do provide excellent possibilities for application of the method. Here are our replies to Dr Cornet's comments.

2 GEOMETRY OF THE FLUID INJECTION (SHAPE OF THE SOURCE)

In principle, for the SBRC the source of the fluid injection need not necessarily be point-like. A point source injection is just a first-order approximation we used to simplify the consideration and data processing.

Of course, the method can be and must be further developed for the case of linear and more complex sources. Moreover, the time dependence of the pressure perturbation should also be taken into account.

Let us now return to the justification of the point-source approximation of the SBRC in the case of the Soultz experiment. Our argument for accepting this approximation was two-fold. First, if we invert in time the evolution of the seismicity cloud and extrapolate the representation of the cloud given in Fig. 3 of SAR99 to time zero, then the cloud will roughly converge to a point, rather than to a line. Fig. 1 of this Reply shows the evolution of the seismicity cloud in time with a time increment of 25 hr. The last view of the cloud is the same as the first one at the bottom of Fig. 3 in SAR99. We consider this figure a good justification of the point-source approximation.

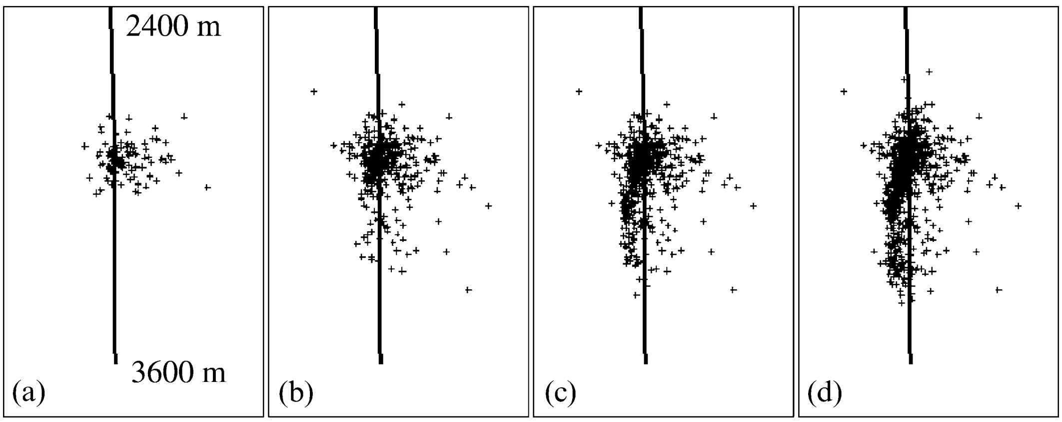

Figure 1. Projection from the south to the north (east is to the right) of the microseismic cloud (a) 25 hr, (b) 50 hr, (c) 75 hr and (d) 100 hr after the start of the injection. The view of the cloud at 100 hr is the same as the left-hand view at the bottom of Fig. 3 in SAR99.

The second part of our argument was that this point ‘approximately corresponds to the centre of the depth interval 2850–3000 m, where flow logs show a major part of the fluid loss (60 per cent; for detailed references see Cornet *et al.* 1997)’—a direct citation from SAR99, p. 210. It was not our aim to describe in details the Soultz experiment. For this reason we gave sufficient (from our point of view) references: Dyer *et al.* 1994 and Cornet *et al.* 1997. For example, in Dyer *et al.* 1994, p. 35, the text reads: ‘Through the main open hole injection test, 93SEP01, it was clear that the flow regime within GPK1 could be split into 3 distinct zones. These were:

Zone 1 2850 m to 3020 m depth Upper Section
Zone 2 3020 m to 3150 m depth Middle Section
Zone 3 3150 m to 3342 m depth Lower Section . . . .

In Fig. 4:5 [of Dyer *et al.* 1994], it is shown that during the September tests the flow regime within GPK1 was dominated by Zone 1, with around 50 to 60 per cent of the flow leaving the borehole within the first 200 m below the casing shoe. Flow in Zone 2 decreased fairly monotonically throughout the test from an initial value of around 30 per cent, to 15 per cent by the end of the injection phase. Flow in Zone 3 was initially low (5 per cent), it then peaked at 37 per cent on 9 September and stabilized at around 20 per cent for the remainder of the test.’

In addition, in Fig. 2 we provide plots of the cumulative depth distribution of the normalized flow rates (modified after Baria *et al.* 1994), showing that approximately 50 to 65 per cent of the flow leaves the well in the depth range 2850–3000 m with different flow rates.

We find that this information is in agreement with SAR99 and with Fig. 1 of this Reply. It also supports our approximation of the injection geometry by a point source.

Of course, any approximation provides biased estimates. To gain an idea of possible bias, we re-estimated the hydraulic

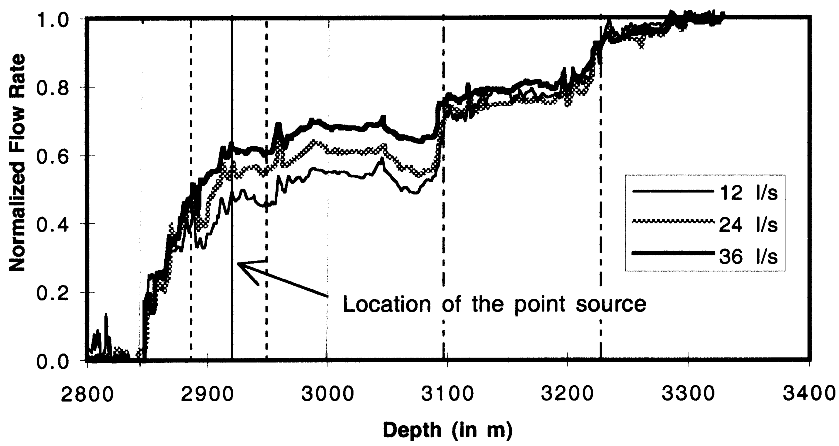

Figure 2. Comparison of normalized flow logs performed during the injection tests at injection rates of 12, 24 and 36 l s^{-1}. Flow rates are expressed in fractions of the total injection flow (after Baria *et al.* 1994). The 12 l s^{-1} injection rate has been reached 5 days (approximately 130 hr) after the start of injection, the 24 l s^{-1} rate 9 days (approximately 200 hr) after the start of injection and the 36 l $^{-1}$ s rate 13 days after the start of injection.

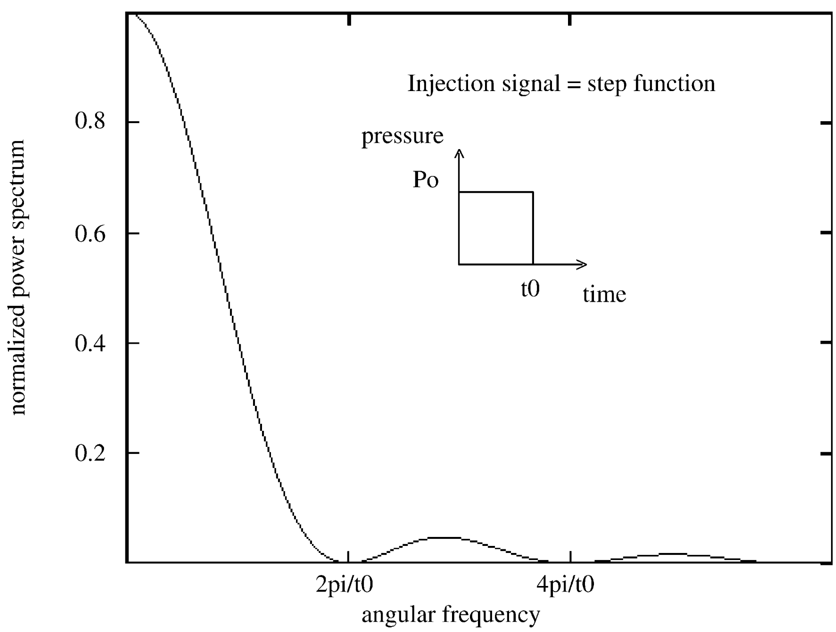

Figure 3. The power spectrum of a step-function-like injection signal cut off at the occurrence time t_0.

diffusivity tensor using two different subsets of microseismic events: (i) the events that occurred during the first 200 hr of the injection (this corresponds exactly to the situation suggested by Dr Cornet of the experiment being performed during the 200 hr after the start of the injection only); and (ii) the events that occurred during the second 200 hr. These estimates are, respectively, as follows:

$$\boldsymbol{D} = \begin{pmatrix} 0.7 \pm 0.2 & 0 & 0 \\ 0 & 1.9 \pm 0.3 & 0 \\ 0 & 0 & 3.0 \pm 2.4 \end{pmatrix} 10^{-2}\ \mathrm{m^2\ s^{-1}}, \qquad (1)$$

$$\boldsymbol{D} = \begin{pmatrix} 0.5 \pm 0.2 & 0 & 0 \\ 0 & 1.6 \pm 0.4 & 0 \\ 0 & 0 & 5.9 \pm 2.9 \end{pmatrix} 10^{-2}\ \mathrm{m^2\ s^{-1}}. \qquad (2)$$

Both estimates are statistically indistinguishable from that given in eq. (16) of SAR99:

$$\boldsymbol{D} = \begin{pmatrix} 0.6 \pm 0.2 & 0 & 0 \\ 0 & 1.7 \pm 0.3 & 0 \\ 0 & 0 & 4.6 \pm 2.4 \end{pmatrix} 10^{-2}\ \mathrm{m^2\ s^{-1}}. \qquad (3)$$

This shows that the bias of the permeability anisotropy estimates should not exceed the error estimates given in these equations.

3 HETEROGENEOUSLY DISTRIBUTED PERMEABILITY

First, our assumptions were related to the hydraulic diffusivity rather than to the permeability. One can imagine a material with a homogeneous hydraulic diffusivity but heterogeneously distributed permeability.

Second, we emphasized many times that the heterogeneous (non-uniform) distribution of the diffusivity does not exclude the possibility of an effective (global) estimate. This is exactly what reservoir engineers try to estimate when performing upscaling. Therefore, our 'large-scale' estimate of the permeability describes the complete heterogeneous seismic active volume in the sense of such an upscaling.

Of course, any heterogeneity of the medium can change such an effective permeability tensor. A principal question here is whether the volume of the studied rock was large enough to be a representative volume for a given reservoir or a geological structure. Evidently, this is a very difficult question for any realistic multiscale heterogeneous geology. We think, however, that the permeability tensor estimates given in SAR99 are sufficiently representative of granitic rocks in the depth range 2500–3500 m on the scale of 1000 m around the GPK1 borehole. This is also supported by the estimate mentioned in the previous section.

4 SHAPE OF THE SEISMICITY CLOUD

Let us start this section of our Reply with a direct citation from Dr Cornet's Comment: 'Hence I conclude that the shape of the seismic cloud is controlled by the fracturing process induced by the hydromechanical coupling rather than by the intact rock mass permeability.'

One of the most important principles of the SBRC is that it is not the shape of the acoustic emission cloud that is important, but rather the velocity of cloud growth in different directions. Thus, instead of the shape of the cloud in the usual (x, y, z)-space, the shape of the cloud in the $(x/\sqrt{(t)},\ y/\sqrt{(t)},\ z/\sqrt{(t)})$-space is of importance.

As described in SAR99 (as well as in Shapiro *et al.* 1997, hereafter referred to as SHB97; see also Shapiro *et al.* 1998), for a given elementary volume, occurrence times of early events are of importance. The later events occurring in this volume do not influence SBRC estimates of the permeability tensor.

To better appreciate this point let us consider Fig. 3. First, we approximate the pore pressure perturbation at the injection point by a step function, which differs from zero until the time t_0 of a particular seismic event. The time evolution of the injection signal after this time is of no importance for this event. The power spectrum of this signal shows that the dominant part of the injection signal energy is concentrated in the frequency range below $2\pi/t_0$ (note that the choice of this frequency is of partially heuristic character; see the related discussions in SAR99 and SHB97). Thus, the probability that this event was triggered by signal components from this frequency range is high. This probability for the low-energy higher-frequency components is low. However, the propagation velocity of high-frequency components is higher than that of the low-frequency components (see SHB97). Thus, to a given time t_0 it is probable that events will occur at distances that are smaller than the travel distance of the slow-wave signal with a dominant frequency of $2\pi/t_0$. The events are characterized by a significantly lower probability for larger distances. The spatial surface that separates these two spatial domains we call the triggering front. Thus, we are interested in the form of the triggering front and not in the form of spatial domains with a high concentration of events occurring much later than the front's passing.

The work of Lockner and Byerlee was dedicated to studying 'how tension and shear failure depend on pore fluid injection rate and differential stress' (Lockner & Byerlee 1977). They performed 18 experiments, only in two of which was the acoustic emission monitored. Moreover, in the first experiment one intact sample of Weber sandstone was fractured due to the slow rate of fluid injection (3.3×10^{-5} cc s^{-1}). For the second experiment, with fast fluid injection (3.3×10^{-4} cc s^{-1}), *another* sample of Weber sandstone was used. Furthermore, the boundary conditions of these experiments were completely different from those of the Soultz experiment: each sample was placed in a polyurethane sleeve to isolate it from the confining fluid. Thus, no fluid filtration to infinity was modelled. The sizes of these rock samples were 19.05 cm long and 7.62 cm in diameter. However, natural stress and pressure conditions were modelled: 1 kbar confining pressure and 4 kbar differential stress. Thus, the spatial scaling of the experiments was very different from that of the Soultz injection. Finally, 'a significant feature of the experiment' of Lockner & Byerlee 1977 (p. 2024) 'is that with the instrumentation used, acoustic emission was not detected until the onset of failure'. Thus, the spatio-temporal domain covered by these experiments was a very small domain located close to the time axis and very far from the envelopes (corresponding to triggering fronts) shown in Fig. 1 of SAR99. It was not the very early low-energy events that were studied, but rather late and strong events.

Because the acoustic emission in the paper of Lockner and Byerlee was used 'to trace the development of hydraulically induced fractures', they plotted not the clouds of events themselves but rather distributions of the event densities at late stages of injections at the same (small) spatial scale as the boundaries of their samples. Thus, we feel that Dr Cornet's argument based on this experiment is irrelevant.

We also think that in both cases, with slow or fast injection rates, our technique would give the same results if it was applied to the same rock sample and to the seismicity cloud including the early low-energy events. We also think that in realistic situations, even in the case of a fast injection, such early events would have the same nature and their triggering front would propagate with the same velocity as in the case of slow injection.

Note, however, that by hydrofracturing, a highly non-linear zone of deformation can exist close to the injection source. In such a zone, the velocity of fracturing, or of the opening of palaeofractures, or even the filtration velocity can be higher than the velocity of the triggering front. In this case the SBRC will not give the permeability of the intact rock. How large such a non-linear zone might be is an open and interesting question. We do not discuss it here; however, we think that in realistic situations its size should not exceed a few tens of metres.

The parts of Dr Cornet's Comment about our description of the seismicity cloud and stress field orientations are also discussed in this section of our Reply. The orientation of the stress field was not a subject that we considered at all. It was given in the paper merely to introduce the reader to the general situation in the Soultz region. The orientation of the stress field is of no importance for the application of the SBRC. It can, however, influence the results of the method, because the permeability tensor can be related to the stress tensor. In the only sentence from SAR99 with information on the stress orientation (p. 210)—which is criticized by Dr Cornet—we wanted (i) to say that the major orientation tendencies of the seismicity cloud are close to the stress orientations, and (ii) to give a non-detailed introduction and some references about the stress orientation. Thus, we provided information from several literature sources (Dyer *et al.* 1994; Klee & Rummel 1993; Cornet *et al.* 1997) and referred the reader to these sources for further data on the stress orientations. Thus, we see no reason to discuss this point further.

In contrast to the stress orientation, the orientation of the microseismicity cloud was also described elsewhere in our paper. This orientation is clearly given by (i) Fig. 3 of SAR99, where the upper part shows the plane view of the cloud and the lower part the projection of the cloud, looking from south to north (east is to the right); (ii) the orientation of the inertia tensor of the seismicity cloud (see p. 212): N170°. This does not differ very much from the orientations of the seismicity cloud that Dr Cornet insists on in his comments.

To summarize, our point of view is as follows. Even in the case of hydrofracturing, in spite of the fact that the shape of high-density zones of seismicity clouds can be controlled by the fracturing process, the growth rate of the outer boundaries of a seismic cloud (that is, the propagation of the first low-energy triggering front) is controlled by the permeability tensor. This is supported by the fact that in Soultz our estimates are in a good agreement with hydraulic estimates.

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